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SYMPOSIUM

Seasick on the Third Wave? Subverting the Hegemony of Propositionalism:

Response to Collins & Evans (2002)

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Harry Collins and Rob Evans (Collins & Evans, 2002) offer a typically forthright normative vision for the 'Third Wave' of science studies, after what they call the earlier waves of post-war rationalism, then the post-Kuhnian 'cultural revolution' from the 1970s. They propose to redefine the accepted qualifications for expert standing in the countless areas of decision-making in which scientific knowledge has held presumptive sway as exclusive (but relentlessly disputed and increasingly eroded) public authority.

Collins & Evans (2002) start from the problem of public legitimacy that has been widely recognized to beset science in recent times (House of Lords, 2000; European Union, 2000).[1] They argue that 'the problem of legitimacy' for science has been mistakenly replaced by 'the problem of extension', in which real distinctions between experts and publics are dissolved and 'technical decision-making rights' (as they call them) are thus extended indiscriminately. Their aim of redefining competences for 'technical decision-making' in the public sphere, so as to include practical experience-based expertise alongside 'certified science', would be more inclusive compared with existing boundaries, but more exclusive compared with the apparent assumptions (of infinite 'extension') of the participation in science 'movements'. They use the case studies of Cumbrian sheep farmers (Wynne, 1992) and HIV-AIDS activists (Epstein, 1996) to illustrate this argument. Significantly, and as issues I take up later, for them the public sphere involves an accumulation of completely unrelated 'decisions' about what they define as exclusively 'propositional' decision-questions, such as whether nuclear power, anti-misting kerosene or UK beef is safe,

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or how to manage sheep farming under conditions of radioactive contamination. Although these issues are obviously relevant, nevertheless this is a seriously impoverished account of what is involved when we address science in public arenas.

As I explain later, I believe their approach reflects a mistaken, if widely shared, understanding of 'the problem of legitimacy' for science in public policy. Their implicit assumption is that the problem of public legitimacy for science is rooted in the way that people with authentic but unrecognized expertise are denied access to expert deliberations about such (propositional) questions as those stated earlier (for example, 'is UK beef safe?'). Their aim is therefore to have this different kind of practical and experience-based expertise recognized and admitted. I suggest instead that this multidimensional legitimacy problem is more about the institutional neglect of issues of public meaning, and the presumptive imposition of such meanings (and identities) on those publics and the public domain. Strikingly, with its exclusive basis in propositional issues, this same omission bedevils the Third Wave too.

Collins and Evans' case for an extended but discriminating normative definition of 'contributory' expertise is incontrovertible; but this is countered by important limitations that I try to explore here. By asking also about how public issues are framed and thus given meaning, I raise neglected questions about how proper knowledge for relatively new domains like 'environmental' and 'risk' problems should be negotiated as matters of 'civic epistemology',[2] and how we have by default allowed previously institutionalized epistemic commitments to be extended to such domains with inadequate collective reflection and debate. My comments thus connect five points: (1) their assumption that public issues in which scientific knowledge is involved are only about propositional questions (the ostensible currency of science), for example about risks, and not also about public meanings (thus inter alia about which propositional questions [and knowledges] are salient anyway); (2) their assumption that the correct entry-point for considering different qualities of knowledge is the scientific core-set as developed by Collins in the Sociology of Scientific Knowledge (SSK), which begs unaddressed questions about what is 'core', and why; (3) their historical vision of the 'second wave' of the field which exclusively centres on internalist sociology of such esoteric sciences; (4) their understanding of public domain processes involving science as composed solely of discrete, unrelated 'decisions'; (5) overall, the risk entailed by their analytical as well as normative commitments, of reinforcing an illiberal cultural imagination based on uncritical acceptance of western scientism.

Realism and Alternation: The Deletion of Context and Meaning

For Collins and Evans, refocusing from truth to expertise and experience still requires a realist orientation, even if its starting-point is a relativist understanding of scientific truth:

> Our question is: 'If [thanks to SSK] it is no longer clear that scientists and technologists have special access to the truth, why should their advice be specially valued?' This, we think, is the pressing intellectual problem of the age. Since our answer turns on expertise instead of truth, we will have to treat expertise in the same way as truth was once treated – as something more than the judgement of history, or the outcome of the play of competing attributions. We will have to treat expertise as 'real', and develop a 'normative theory of expertise'. (2002: 237)

Curiously, this seems to imply a return to the First Wave, now treating expertise as 'real', instead of scientific knowledge alone. As with many other such debates, it is entirely ambiguous what they mean by 'real' here. Of course we can agree that expertise is real, but its salience, validity and authority with respect to a public issue are still conditional: these conditions can be elicited with SSK 'methodological relativism' (Collins, 1981). This raises the question of what we think this reflexive scepticism is for. Is it: (1) to be a participant in the contestation of propositional truth about, say, the consequences of this-or-that technology or the existence of paranormal forces; or (2) to raise questions concerning how existing propositional knowledge-claims ('realist' discourses) also harbour and project tacit commitments to particular meanings or salient questions, which if just presumed and left implicit can become effectively imposed with no collective negotiation?

Whereas my vision is more towards the latter, Collins and Evans appear to assume the former.[3] As such, they have to adopt an essentialist epistemological stance; this seems to be wielded too inflexibly. Here, we find interesting echoes of the epistemological chicken 'bath-water' debate between Collins & Yearley (1992a, 1992b) and the Paris school of Callon & Latour (1992).

In that earlier critique of the Paris version of the reflexive turn, Collins and Yearley proposed to keep scepticism, most especially the semiotic variety of Latour and colleagues, on a very tight, possibly suffocating leash; otherwise it 'leads us to have nothing to say ... the result is impotence' (Collins & Yearley, 1992a: 302–03). Instead of infinite epistemological regress towards self-extinction, akin to the street-game of chicken, Collins and Yearley advocated 'meta-alternation' as a pragmatic necessity, a universalistically-prescribed boundary between essentialist commitment to 'real' categories, like 'nature', 'the human', or in the present case, 'expertise', and constructivist scepticism.

However, recognizing that realism of some kind is unavoidable does not mean adopting the kind of essentialism advocated by Collins & Evans (2002). To do this is to delete the conditionality of 'real' expertise. Latour's original insight was seminal, that laboratories can be seen as the 'centres of calculation', or 'obligatory passage-points', for distant world-making (Latour, 1983, 1987). Yet even so, most of the laboratory studies, which were defined as the SSK mainstream from about 1980 onwards (for examples, see Latour & Woolgar [1979, 1986], Collins [1985], Knorr

[1982, 1995], Lynch [1985], Traweek [1988], Pickering [1984] and Pinch [1986]) were not concerned with such externalist questions.[4] When one builds the frame and field of view of 'expertise' (or the same, science in public) implicitly upon internalist sociology of esoteric laboratory sciences and their core-sets as sociological object, as Collins & Evans (2002) unequivocally do, the issue of how one's SSK analytical role relates ambiguously to one's role as 'knowledge-actor' in a public arena may appear less significant. I suggest that this has inadvertently occurred for them. The wider issues of salience and meaning are almost invisible from this starting-point.

Thus, they seem to have confused the narrow confines of laboratory sciences SSK with the fundamentally different further issues raised for scientific knowledge in public arenas, which is the focus claimed for their 'Third Wave' programme. Correspondingly, they seem to be playing the role of contributor to such 'realist' propositional discourse. There is a significant general difference between the kinds of process in which scientific laboratories make their interventions in the world outside through technological artefacts and their associated disciplines and consequences, and those in which the wider interventions occur and recur through discursive networks and narratives of scientific knowledge for policy, such as in 'risk management' public policy issues and decisions. As studies of the latter kind of issue emphatically demonstrate, contestation is rarely only about propositional truths, but is more usually also, if more obliquely, about what is the proper public meaning and definition of the issue(s) being contested (Nelkin, 1992; Martin & Richards, 1995; Irwin & Wynne, 1996; Grove-White et al., 1997; Hilgartner, 2000). Of course, these dimensions interpenetrate and encompass questions of the legitimacy of whatever social institutions are claiming authority.

Collins and Evans' assumption of a 'contributory', propositional role, and its essentialist implications, corresponds with a neglect of context and a denial of the ultimate contingency of saliency and meaning. This undue reduction and essentializing of frame is also evident in their presumption that for such public issue definitions, science is anyway the proper, 'natural' frame of reference. They thus define the public domain to be only about whether or not something is true. They entirely ignore that public policy processes, and public reactions to scientific discourses of intervention in, and attempted management of nature and society, are processes of (often implicit and oblique) negotiation of public meanings. This 'negotiation' may not be visible because it often occurs by default in more authoritarian mode, as citizens experience the presumptive, non-negotiated imposition of scientific frames of meaning on those public issues and their public actors, by powerful expert bodies. Yet by ignoring these dimensions and building their perspective solely on the negotiation of propositional truth, Collins and Evans risk reinforcing in practice just this authoritarian social idiom, in which public meanings (and identities) are not problematized, but presumed and imposed.

Science, 'Civic Epistemology' and Public Meanings: Scientism Rules?

Collins & Evans (2002: 236) justifiably advocate a return to 'old-fashioned' epistemological questions 'about the grounds of knowledge'. However, they interpret this to mean only questions of, 'is it true?', neglecting questions about the social purposes and objects of knowledge. Addressing this question is a precondition for defining valid public knowledge ('sound science' in the fashionable policy parlance) (May, 2002) and for defining valid trajectories of scientific and technological innovation (Wynne, 2002). Domains such as the new biosciences, being intervention as well as 'innocent' representation (Hacking, 1983, 1992), and generating unpredictable consequences beyond prevailing scientific knowledge, have attracted just such reasonable questions from ordinary publics about the human purposes driving them. They have received few answers from the institutional experts, who have presumed the meaning of the issue to be 'safety' and have thus ignored other dimensions (such as institutional-scientific arrogance), which different publics evidently regard as core (Wynne, 2001; Wynne et al., 2001).

These questions of what are the issues, and what kind of knowledge is in principle salient, arose in my sheep-farmers case study, which Collins & Evans (2002) use; but it may be significant that they did not recognize this, because they do not appear to recognize that issues of public meaning or framing of the issue are open, and usually disputed, before we reach the propositional questions about risks, benefits, and so on, which they assume automatically to define the 'core' issue.[5] In effectively presuming such questions to be inconceivable, they follow what scientific actors in public policy issues are often seen to do, which is to presume the core meaning as if this were objective and given, and then to impose it on the public without question. This denial of the ontic (cultural) politics of science (Verran's, 1998, apt phrase) is where the charge of scientism bites, beyond possible questioning of and expertise with propositional claims.

The point is that hermeneutic openness, variability and multivalency prevail within the scope of what is often called the scientific element of science-intensive public issues, as well as within what is recognized as the public element. That, after all, is the regular fare of science studies! Indeed one effect of realist discourse is to delete domains in which meanings might be seen to be in question, and this deletion is, by default or by intent, part of the tacit social negotiation of just these boundaries. In this respect Collins and Evans may be, in effect, partisan players in such boundary negotiations.

Recognizing these omitted issues involves questions of how definitions of public issues are established and maintained, and thus what becomes salient and what is deleted from collective attention. Questions of issue-framing come into focus. Examining some relevant cases demonstrates how these framing processes also beg questions about the usually black-

boxed ambiguities in the epistemological criteria that are deemed 'natural' for defining sound knowledge, or sound science, for a public 'issue'.

The 1977 public inquiry into the then-proposed thermal oxide nuclear fuels reprocessing plant (THORP) at Sellafield–Windscale, and the 1994–95 case of the Brent Spar North Sea oil platform exemplify these points.

The THORP Issue: 'Plutonium Economy' or Single Plant?

When British Nuclear Fuels proposed THORP at the 1977 Windscale public inquiry (Wynne, 1982), objectors argued that it was meaningless to assess the risks and implications of this one plant alone. THORP would produce plutonium to be used, it was then assumed, for nuclear bombs or for future fast-breeder reactors, and this plutonium would be regularly transported around the world along with spent fuel, nuclear wastes and other dangerous materials in a so-called global 'plutonium economy'. THORP would also hugely increase nuclear waste production, yet with no established means of disposal. Thus, objectors argued that the risk assessment of THORP must logically encompass those wider consequences of its construction; it was utterly artificial and misleadingly reductionist to confine assessment only to the immediate effects of that particular plant at that particular site. Foreshadowing current preoccupations, critical environmental analysts (Flood & Grove-White, 1976) included questions about terrorist hijacking and use of such plutonium, and about the security culture that would inevitably attend these developments. The inquiry chair dismissed this attempted issue-framing as emotive and irrational, because it was so speculative and ill-defined.

Yet it was difficult to deny that these further interconnections were indeed real possibilities, even if not precisely and deterministically specifiable. However, the chair took the analytical and epistemological route already institutionalized and taken for granted in the prevailing policy culture, one with which his legal training also naturally resonated. Sound science and rational policy demanded precision, and precise questions that implicitly reflected controlled empirical observation and analysis. This in turn reflects the historical domination of an implicit laboratory physical science model at the apex of the accepted hierarchy of knowledge, one in which controlled observation of relevant variables takes precedence over comprehensiveness and realistic simulation of the less controlled and more complex factors and situations outside the laboratory. There are good reasons within classical science for this epistemological commitment to precision and control before realism and comprehensiveness.[6] But when the issue is how to assess the pros and cons of such a development as THORP, it is also reasonable to recognize responsibility for the larger consequences, and this different meaning implies a different set of criteria for defining sound knowledge, as the purposes of that knowledge have shifted. Instead of acknowledging this as a matter to be debated, however, the chair translated all such concerns into the established reductionist and

precise cultural meaning, deleting those cultural and epistemological questions from view.

There was throughout these interactions, a tacit conflict between incompatible versions of both the proper definition of the issue to be resolved and the purpose of the prevailing knowledge, and thus of which criteria should define sound science or knowledge: (artificial) precision and control, or 'realism' and comprehensiveness? Both had arguments in their favour. Arguably, there should have been a public debate about this framing question, and about its corollary: what should be the proper epistemology for major public issues of this kind, which also involve science?

The Brent Spar Controversy: Precision versus (Social) Realism?

The 1994 controversy over the North Sea Brent Spar oil platform (Rose, 1998), which had been decommissioned by Shell, shows similar sociological, political and 'civic epistemology' issues. In accordance with UK government policy assessment, Shell were towing the disused platform to dump it on the North Atlantic ocean floor, when Greenpeace activists took it over, with a media campaign to highlight such 'irresponsible' dumping. Facing a rapidly escalating Europe-wide consumer boycott of their petrol in support of Greenpeace, Shell rapidly decided to abandon their plans, leaving the UK government alone to defend the discredited 'sound science' of their dumping policy. Although Greenpeace was later accused of deliberately misrepresenting the environmental risks posed by the Brent Spar (due to incorrect analysis of toxic wastes in the platform sump), this particular mini-issue was allowed to obscure the more fundamental one, which was that Greenpeace and the many people who supported them, were arguing that the salient environmental risk question was not that of the single platform alone. It was the first of 400 or more such platforms that would all soon be decommissioned from the North Sea. If these also were dumped, they could become precedents for all sorts of other wastes too, including UK nuclear waste-inventories that had been slated for ocean-bed disposal.

Here again we can see conflicting framings of the scope and meaning of the issue, and of the risk-analytical problem, corresponding with conflicting epistemic principles of sound science and rational assessment. The propositional issue of 'is it safe?' has to be accompanied by definition of what 'it' is, as well as what 'safe' is. There might also be further legitimate questions beyond environmental safety, even of the larger possible environmental burden. Shell and the UK government operated with the conventional cultural and epistemic commitment to precision and intellectual control, thus to the narrowest possible definition of the question of salient cause (one rig only) and consequences. They dismissed any further disposal questions as irrelevant, too nebulous and imprecise, subject anyway to future independent assessment and decision by (to them) trustworthily impartial authorities (the same institutions). Objectors, disinclined to trust

such institutional impartiality, demanded that the realistic and responsible question was the more comprehensive one, because 'if we let the first one go this way, experience says the rest would almost certainly follow'. They thus posed the more realistic and comprehensive framing. This inevitably meant a less precise set of questions. Again, as with the THORP issue, the epistemic conflict is apparent[7] between precision and realism or comprehensiveness as the basis for public knowledge.

In the Brent Spar case, again the established culture that reflects the conventional implicit hierarchy of knowledge, precision and control was automatically enacted, but this invites questioning that goes to the heart of conventional unquestioned notions of sound science.

The relevance of this for Collins & Evans' (2002) Third Wave concept is that they have said nothing about the importance of context and questions of meaning for such public issues. Such silence is consistent with their assumption that the correct approach to public arenas of science is via the esoteric laboratory sciences, with the associated concept of the core-set as the unquestioned social and epistemic (and hermeneutic) yardstick. Thus, they embed the presumptions of the prevailing epistemic culture, reproducing narrow definitions of public meaning as if they are objective and natural. Their approach thus deletes any social and epistemic questions about how public meanings, issue-framings and definitions of the focus and framing of 'proper' knowledge, are established and presumptively imposed. By default, they thus subscribe to and reinforce a questionable partisan version of these questions rather than reflect upon them, or raise possible alternatives and their different implications. My different vision of the proper role of SSK here would be to try to articulate what a more inclusive social debate over knowledge and its proper grounds and human purposes should be, and how this would open up spaces, now colonized by existing scientific culture, to collectively negotiable questions of public meaning.

In attempting to explain these reservations about the Third Wave in terms of its confinement to questions of who should decide propositional truths, I am suggesting that such restricted matters bear larger meanings, sometimes explicit, sometimes not. The Cumbrian sheep-farmers case study also illustrated this, even though Collins and Evans ignored this aspect. We should also note that even had the farmers' (limited) specialist propositional knowledge been recognized and used by the scientists, for example, knowledge about local variations in environmental conditions beneath the resolution of prevailing scientific spatial units, the whole relationship was one in which the power to define the meaning of the questions remained with the institutional experts: scientists and officials. Thus, even though the farmers rejected some official meanings, they rarely openly challenged them, and the dominant issue-definitions, which affected what kind of knowledge was recognized as salient, remained unscathed on the face of things, whilst being dismissed wholesale by the farmers more privately.

In other words, it seems as if Collins & Evans (2002) have made a discrimination that is incontrovertible as far as it goes, but how far it goes is in question. Our culture has successfully domesticated and (so dominant institutions assume) 'closed' many questions of issue-definition and meaning under the institutional presumptions that these questions never existed, and that the issues are 'scientific', where we should first gain the facts (for example 'what are the risks?') and only then allow 'human values' to be voiced.

To elaborate on the civic epistemology issue noted earlier, the sheep-farmers case also showed how different normative concepts of the proper social role and expectations of knowledge, as embodied in incompatible practical cultures, also separated the Cumbrian farmers and government scientists. Whereas the scientists took for granted the dominant institutional commitment to prediction and control, the farmers took for granted a lack of control and the unpredictability of many important variables, and thus regarded any form of knowledge and practice based on prediction and control as suspect ab initio, regardless of which truth-claims it might make or deny. Moreover, they found such normative assumptions not only to be practically inadequate, but also morally unsound and having wayward meaning. In this case too, there might usefully have been a debate about the proper epistemology for such an issue, and this could have affected the legitimation problem for the better. The sheep-farmers case seems typical of many others, including those derived from developing countries in which relevant cultural divisions are usually deeper (Hobart, 1993; Vitebsky, 1995; Watson-Verran & Turnbull, 1995; Leach & Mearns, 1996; Scott, 1998; Verran, 1998, 2001; Ellen et al., 2000; Agrawal, 2002). It also seems to underline the point that the problem of public legitimacy for modern science is more about its presumptive performance of alien public meanings and civic identities than just about the exclusion of legitimate actors from propositional debates.

The Public Domain as 'Decisionist'

Misled by their twin frame of core-set conceptual foundations and propositional issues, Collins & Evans (2002) prosecute the assumption that the issues are science-centred in terms of their meaning, not only in terms of knowledge questions and actors. This 'core-set' framing, and their unquestioning imposition of what is deemed core and what marginal, combines with and reinforces their assumption that the issues for which science is constructed and deployed in public arenas are indeed 'technical decisions'. After counting seven occasions in the first three pages, I stopped noting the number of times their paper described the public issues as 'technical decision-making'. Their definition of the public sphere in which science plays a role is itself significant:

> By 'technical decision-making' we mean decision-making at those points where science and technology intersect with the political domain because the issues are of visible relevance to the public: should you eat British

> beef, prefer nuclear power to coal-fired power stations, want a quarry in your village, accept the safety of anti-misting kerosene as an aircraft fuel, vote for politicians who believe in human cloning, support the Kyoto agreement, and so forth. These are areas where both the public and the scientific and technical community have contributions to make to what might once have been thought to be purely technical decisions.[8] (2002: 236)

Collins and Evans reduce public policy and the larger public arena in which negotiations of meaning, authority and legitimacy take place, to what Habermas (1975) long ago critically dismissed as decisionism. This is a model in which policy and political processes are conceptualized exclusively as a series of completely unrelated specific decisions, each one of which has no interaction with any other. This deletion of context and atomization of the public policy sphere allows Collins and Evans to imagine that the scientifically dense issues that arise in the public domain have objective meanings, which are not themselves problematic in terms of their origins or substance. It is such presumptive imposition of public meanings and corresponding identities – 'imagined community' (with imagined and imposed public meaning) to use Anderson's (1989) felicitous phrase – that I have argued is a central component of the role of science as the culture (in the sense of agent of meaning) of modern policy, not only as intellectual resource. It is also over this neglected distinction between science as culture (scientism) and science as resource that Collins and Evans' phrase about acceptance of 'living in a Western scientific society' is so deeply ambiguous, and their particular chosen meaning so problematic.

If we do not recognize that there is an issue about science's de facto role in modern society as assumed agent of the meaning of public issues (as if these public meanings were objective, given and universal, for example as 'risk issues'), then we are left thinking that the issues are indeed 'technical decision-making' only (with no wider human agency in question in their prior framing), with correspondingly defined core-sets and corresponding 'properly' marginal social and intellectual hinterlands. If on the other hand we accept that often at issue are also the prevailing framing of the meaning of the issue, then we immediately see that the legitimacy issue is not just about who qualifies as competent to participate by reference to some expertise that relates to the presumed question, but also about how dominant actors have illegitimately excluded people from negotiating what the salient questions are in the first place. In this sense, 'context' may become 'content'.

Thus, Collins & Evans (2002) fail to see that the public domain issues are about the democratic cultivation and negotiation of public meanings through the continuities and interwoven textures of public experiences, relationships, knowledges and interactions, in essentially open-ended historical form. The public domain is not just a kaleidoscopic accumulation of atomistic, ahistorical public decisions based on expert knowledge, however inclusively we might define 'expert'. A more relational vision is needed.

In this sense also, the otherwise useful distinction that Collins and Evans make between specific experience-based practical competences and more codified scientific expertises needs to be qualified. To the extent that public meanings and the imposition of problematic versions of these by powerful scientific bodies are the issue, then the proper participants are in principle every democratic citizen and not specific sub-populations qualified by dint of specialist experience-based knowledge. Moreover, the participation of these citizens is indeed in the science as so defined, since this is institutionally defined in such a way as to have presumptively encompassed and colonized such broader realms of meaning under the defined umbrella of 'science'. Again we encounter the issue ignored by Collins and Evans, of problematizing the prevailing boundaries of 'the scientific' and 'the public' (or 'the political'). Their approach would reinforce the self-deluding institutional trap from which public rejection can only be understood as due either to wilful rejection, or misunderstanding of science, not as a rejection of arrogant and illegitimate institutional presumption of public meanings. A crucial dimension of what should be democratic discourse, debate, and negotiation involving science is thus deleted and pre-empted. I am unsure whether Collins and Evans mean to defend this.

Conclusion

I have argued here that science has been allowed by default to be the agent of public meaning, in the ways that dominant institutions presume public issues to be 'scientific', and often particularly reductionist versions of 'scientific' too. Of course it can be countered that, as is well attested, powerful bodies use science deliberately to advance other covert agendas. This has to be acknowledged; but it can be said to sustain a situation in which, rather eerily, public discourse with which no-one identifies is being used. An alternative is that, perhaps using science more and more in this instrumental way, we have as usual become entrapped and encultured by our own contingent constructs. This ambiguity continues. In favour of Collins and Evans it could be argued that, in practice, questions of meaning can or should be ignored because the discriminating democratization of one-dimensional propositional deliberations (if such could even be imagined as a separate sphere) would naturally be accompanied by democratization of the negotiation of public meanings. However, this would need at least to be recognized and argued through, with the benefit of empirical illustration. It would be wholly inadequate to argue that this issue of negotiation of public meanings is a separate, political matter, because those very boundaries are themselves a key issue, and because 'negotiation' of meanings and frames is usually so embedded as an oblique, tacit kind of un-negotiated 'negotiation', within those ostensibly scientific, propositional public discourses and interventions.

This issue continues to confound the gargantuan political and scientific efforts to 'close' the relentless dissent over genetically modified crops

and foods in Europe. Whilst public opposition is defined as misunderstanding or wilful rejection of science, it is more inspired by public alienation from the responsible institutions' insistently imposed frames of meaning, including the convinced view that public reactions are framed within the meaning of risk as defined by science. In the UK, extra farm-scale trials were reluctantly initiated by the UK government in 1998, under mounting pressure from critical scientists and environmental non-governmental organizations. These trials elaborated the existing European Union regulatory testing process composed of step-wise progressive escalation of risk assessment. If evidence from the preceding step showed no reason to stop, the process proceeded from laboratory bench, through greenhouse then controlled field tests, to full-scale commercialized agricultural uses. The approach of Collins and Evans fully corresponds with what we see in the institutional handling of this issue. The whole public meaning is seamlessly and unquestioningly defined as a propositional scientific question of risk that the new farm-scale tests will answer. Thus, UK government statements have repeatedly falsely asserted, with no public contradiction and implicit support from the scientists, that these new scientific tests (due to be completed in the autumn of 2003) will provide the definitive evidence, one way or the other, for closure of the issue (Agriculture and Environment Biotechnology Commission, 2001).[9] Yet this reduction of the issue to one of science alone, consistent with the Third Wave approach, deletes altogether the many other dimensions of the genetically modified crops issue which people define as its meaning. These include the problematic institutional culture that routinely defines public issues involving science in this scientistic way (Wynne, 2001; Wynne et al., 2001), and that routinely exaggerates the power of science through risk assessment, to anticipate and control future consequences. In presuming to impose such public issue-meanings, this also provocatively imposes deeply alien identities and values, and ontologies, on people. It is thus hardly surprising that institutional science has a legitimacy crisis; but this is not a legitimacy crisis caused only by the exclusion of unrecognized but legitimate forms of expertise from propositional negotiations about consequences. It is a legitimacy problem caused more by the undemocratic imposition of assumed meanings on the issue, and on the public, through the institutional scientific culture. Collins & Evans (2002) thus misunderstand the nature and causes of the legitimacy crisis of science. In implying that science also properly defines the meaning of the issues, they reinforce western scientific society's scientistic cultural hegemony. Whichever waves it may be riding, science studies surely has a more challenging role to play than this, to help reassert what are quintessentially cultural–political questions sequestered by the culture of science, about public meanings and saliency.[10]

It is very difficult to effect a boundary between 'negotiation' or dissemination of meanings and frames, and the resolution of propositional issues, since conflicts apparently over the latter often also embody differences over the former. Powerful agents can define public meanings in ways

that pre-empt negotiation, but that may still be refused, explicitly or informally, by those who were excluded. Whether the meanings–saliency issues need to be resolved first, or to be resolved along with the propositional issues, we cannot ignore this major dimension, as Collins and Evans have done.

Collins and Evans' prescription for meta-alternation reflects their prior commitment to fixed versions of the social, and thus of the natural, which in turn corresponds with their framing of the issues for science in public as proposition-based only. Yet it seems an essential responsibility for SSK to explore and put into question just these given boundaries, and presumptively imposed meanings, for society at large to address and learn to negotiate.

We can surely sustain a respect for real expertise, scientific and non-scientific, which remains conditional, for example upon what the issues at stake, and the aims of knowledge are defined to be, as well as on judging its validity against whatever epistemic criteria are in play. Collins and Evans' form of realism seems to demand unconditional surrender to dominant, often scientistic, frames of public meaning. This would also involve abandoning commitment to real practical collective exploration of better human relational possibilities, as woven in with our knowledges. I think it appropriate to maintain a tension between what we might imagine as deeply entrenched and maybe 'inevitable' social–cultural realities of modern society, and what (with help from our analytical repertoire) may be worthy of reimagination.

Notes

1. This is more of a European than a North American problem it has to be recognized, and even in Europe a highly uneven one.
2. The term 'civic epistemology' should be credited to Sheila Jasanoff, who has used it in various recent presentations, but of course she bears no responsibility for the specific meaning I have given it herein. I have earlier used the term 'public epistemology' to capture the same thoughts (Wynne, 2002).
3. Collins and Evans introduce the useful category of 'interactional expertise', mediating between 'certified' experts and 'uncertified' experience-based expertise, but then define it again only in terms of an expertise in helping unrecognized experts like the sheep farmers to have their propositional expert knowledge included; I tried, however, to represent the different and unrecognized issue-meanings held by marginalized groups, such as the farmers, to counter their neglect by institutionalized experts and policy bodies. Of course, this can also entail trying to have their (propositional) knowledge recognized (if they have any such), but it is much more than this alone. I have tried to achieve this same role in the UK and international nuclear issue, the post-Chernobyl sheep-farmers case and the current genetically modified crops issue; however, this has involved trying to represent the meanings of not just specialist experience-based public 'expert' groups like farmers, but simply wider public meanings, questions and issue-definitions excluded from the institutional expert culture. This is a different vision from that of Collins and Evans of the public 'interactional' role of SSK actors like myself, though I am not suggesting that any single such role-model should be sovereign.
4. Though they did not deny them. It was more that the new internalist sociological programme was demanding enough, and left the external dimensions for another day.

5. There is a striking parallel here with social science research on 'public perceptions of risk'. This field's very definition reflects the assumption that the natural and universal object of those public attitudes is 'risk' (as defined by the prevailing science). Some mainstream social scientific work on 'risk perceptions' (for example, Midden et al., 2003) explicitly discusses 'core' content of such attitude-objects, and 'context', as if these were naturally given distinctions for public meanings to reflect. Yet my own work has suggested that, for example, endemic social dependency on powerful institutions and thus 'social risk' arising from that dependency, could be a 'core' object of typical public concerns, not a secondary 'contextual' dimension that reflects lack of 'factual' ('core') knowledge (Wynne, 2001). Social science framings of risk often reproduce existing scientific framings of core content and secondary context; indeed they often seem to reflect assumptions of what is measurable and scientifically definable and, therefore, objective core content, whilst those questions (elements of meaning) which are less precisely definable are therefore only secondary context. This corresponds with the present discussion of civic epistemology. Such social scientific constructions and performances of public meaning are scientistic, and are paralleled by Collins and Evans in their restriction of meaning to propositional questions.
6. Though it is worth recalling Kuhn's (1970) point that in practical scientific cultures there is always ambiguity and negotiation as to the specific situational conflicts between the accepted scientific epistemological norms of precision, scope, symmetry, elegance and accuracy.
7. Campbell's (1985) case study of the conflicts over the Canadian McKenzie Valley oil pipeline described a similar conflict between precision and comprehensiveness in the preferred implicit epistemologies of the competing scientific views. This 'propositional' conflict also thus expressed different meanings, and different epistemic criteria.
8. The end of this passage hints at a less exclusively technicist understanding of such issues than that for which I have criticized them. It is thus unfortunate that nowhere in the paper do they give this bare hint any airing, and indeed otherwise in effect dismiss it.
9. The Agriculture and Environment Biotechnology Commission subcommittee, which produced the 2001 *Crops on Trial* report that criticized this exaggeration of the scientific meaning of the farm-scale trials, did contain two scientists amongst its six members, but the criticisms were included despite the opposition of those scientists. A typical policy representation of this science was that 'these farm-scale evaluations will ensure that the managed development of GM [genetically modified] crops in the UK takes place safely' (Department for the Environment, Food and Rural Affairs, 1999).
10. As so-called Wave-Two science studies authors such as Jasanoff (1990), Ezrahi (1990) and Wynne (1982) have shown years ago, in practice the 'negotiation' of public meanings takes place not before the conduct of propositional issues, but tacitly through that very process itself. The literature on scientific knowledge as policy heuristic (Van der Sluis et al., 1998) also suggests that epistemic norms and other elements of collective public meaning are being tacitly shaped and negotiated as apparently only direct propositional questions are being addressed, such as what will be the average global sea-surface temperature rise for a doubling of greenhouse gas concentrations in the atmosphere? As feminist science studies in particular have highlighted (Singleton & Michael [1993]; see also the *Social Studies of Science* special issue [2002], 32[5/6], October–December on 'Postcolonial Technoscience'), this multivalency issue in scientific knowledge is much more pervasive and important; this issue connects with similar anthropological debates about practical knowledge cultures, indigenous meanings and external representations (Ingold, 2000; Agrawal, 2002; Franklin, 1995; Goodwin, 1994; Reckwitz, 2002).

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